In an introduction a brief historical survey is given of our knowledge of the Jurassic plants of Yorkshire, and also of the Jurassic plant-bearing strata of France, Germany and other countries which resemble those of the Yorkshire coast.

In the descriptive part of the catalogue the specimens are grouped, so far as possible, in accordance with their natural affinities. Fifty-five species are described, and are distributed as follows:—Bryophyta, 1; Equisetales, 2; Filices, 20; Cycadales, 23; Coniferæ, 9. There is a resemblance between this flora and the Wealden flora, among the common characteristics being the absence of Angiosperms and abundance of Cycads and Ferns.

In conclusion, Mr. Seward remarks: "It is in the southern tropics that we must look for existing forms which afford the most striking links between the vegetation of to-day and that which has left imperfect records in the Jurassic sediments of the Yorkshire coast. The climate was presumably more tropical than that of North Europe at the present day; there is no evidence that the plants of Jurassic times grew under conditions which induced xerophytic characters, moisture being probably abundant and favourable to the luxuriant growth of Equisetums and Ferns."

*Practical Electrical Testing in Physics and Electrical Engineering.* By G. D. A. Parr. Pp. vi + 392. (London: Longmans and Co., 1901.) Price 8*s.* 6*d.*

Mr. Parr is head of the electrical engineering department of Yorkshire College, Leeds, and the book before us represents the instructions and experiments given to students in the practical course in that department. It is not our object to criticise that course, the value of which must largely depend on the theoretical teaching accompanying it; suffice it to say that it is apparently modelled very closely on that given by Prof. Ayrton at the Central Technical College. Whether there is sufficient justification for the publication of Mr. Parr's book must depend on whether the details of the system and apparatus are sufficiently widespread; in the majority of cases the instructions given for each experiment imply the provision of special apparatus for carrying out the test, and the instructions are given, if we may use the phrase, in terms of that apparatus. A more general description of the experiments would be of wider use, though probably not so convenient for the student actually passing through Mr. Parr's course. There is a good appendix describing the chief instruments and apparatus used, and another appendix giving solutions of the various problems raised by the experiments which we do not think so valuable, except as a labour-saving device to the demonstrator or idle student. The experiments are, on the whole, well devised to bring out clearly the fundamental laws of electricity and magnetism. We are sorry, however, to see included an experiment to "prove" Ohm's law, in which P.D. is measured by a high-resistance galvanometer; a galvanometer can only be employed to measure P.D. if Ohm's law be true, so that it cannot logically be used for this purpose in an experiment to prove the law. The method given second by Mr. Parr, in which an electrometer is used to measure P.D., is the only satisfactory one for proving Ohm's law. Technical teachers who are seeking to develop a practical course will find this volume a valuable guide.

## LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* Nature. *No notice is taken of anonymous communications.*]

### Audibility of the Sound of Firing on February 1.

It is an interesting question how far the accounts of various observers as to the sound of the minute guns on February 1 having been distinctly heard at many distant stations and not heard at others comparatively near by attentive listeners, and as to the character and duration of the sound, can be explained by known laws of the propagation of sound in the atmosphere. To this question I should like here to offer such answer as I have been able to arrive at after careful consideration and some rough calculation.

The firing line extended from the *Majestic* at the eastern end to the *Alexandra* at the western, in a direction some 6° N. of W. for about 8 miles. The eastern half was a double line of 16 pairs of ships, the distance between the two lines being about ¼ mile, nearly the same as that between the successive ships in each line (2½ cables or ¼ sea mile); while the western half was a single line of 14 ships.

Some stress has been laid by observers near the firing line on the want of simultaneity in the discharges from the different ships. There is doubtless need of more accurate information on this point, but I cannot help thinking Mr. Hinks's estimate that, as "the firing ran down the double line, the interval between the successive pairs of flashes was about half a second," is excessive. This would add, to an observer at Southsea, 8 seconds over and above the 19 to 20 seconds by which the sound of the westernmost pair of the double line was necessarily behind that of the first pair, whereas he says that the whole sound "lasted only about 20 seconds." I do not think, therefore, in the absence of more exact information, account need be taken in a general explanation of the supposed want of simultaneity, which I estimated on such information as I could get as not amounting to a difference of more than 2 or 3 seconds.

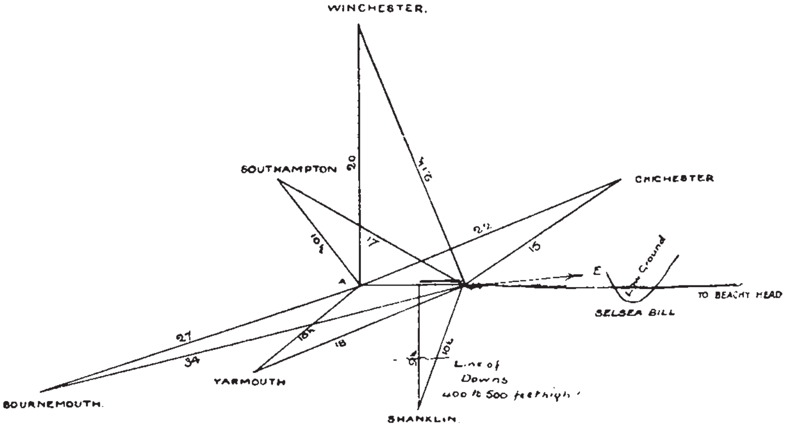

Fig. 1

Mr. Hinks's observation is in another respect important, as it would seem to show that the second half of the firing line was unheard at the distance of 4 to 5 miles from its eastern end.

The known causes which would or might affect the audibility of the sound impulses arriving at any station are, I think, these: the position of the station relatively to the firing line, the direction of the wind and the variation in its speed with the height above the earth's surface, the variation in temperature of the different strata of the atmosphere, and for greater distances the curvature of the earth's surface.

Of these, for distances not exceeding 30 to 35 miles from the firing line, I believe the dominant factor to be the first, that is, relative position of the station.

The accompanying diagram (Fig. 1), drawn roughly to scale, shows this, I think, conclusively.

At Shanklin the reports were heard, but they were not very loud or at all comparable with such as are heard there (and felt too) when gunnery practice with big guns is going on near Portsmouth. This is what might have been expected, since the nearest distance from the firing line was $9\frac{1}{2}$ miles from a point close to the west end of the double line of ships, and the distance from its extremities about $10\frac{1}{4}$ miles. Hence the sound impulses from the whole line arrived within the interval required by sound to travel $\frac{3}{4}$ of a mile, or about $3\frac{1}{2}$ seconds, and thus concentrated were audible, although a line of downs 400 to 500 feet high intervened at a distance from Shanklin of about 4 miles.

At Yarmouth a gentleman, who went on to the pier specially to listen, heard "not a single gun." This is explained by the fact that he was so near the prolongation of the firing line westward that, though only $10\frac{1}{2}$ miles from the western end, he was 18 miles from the eastern extremity, and so the sound impulses, spread over an interval corresponding to $7\frac{1}{2}$ miles, or about 36 seconds, must have arrived at intervals of a little more than a second as separate sounds from the successive ships, each by itself too feeble to be audible.

The same explanation holds for Bournemouth west and a little south of the prolongation, and for Chichester east and a little north of the same, where the sounds were not heard, the impulses being spread over an interval of about $33\frac{1}{3}$ seconds.

Winchester is 20 miles nearly due north from the western end of the firing line, and its distance from the eastern end is about $21\frac{1}{2}$ miles. The difference of $1\frac{1}{2}$ miles corresponding to about 7 seconds might perhaps have been expected to have produced sufficient concentration to make the reports audible, but they were not heard, and probably this is partly due to the intervention of the downs at the foot of which Winchester lies.

As to Southampton I have no information, but as it corresponds in position to the N. of the line almost exactly to Yarmouth on the S., there can be little doubt but that there too the firing was inaudible.

Thus far an explanation founded on the supposition of a still atmosphere of uniform temperature appears to be sufficient. But we have good evidence of the reports having been distinctly heard at distances of sixty, seventy, and even to seventy-five miles from Portsmouth in directions varying, from the eastern prolongation of the firing line, northwards through north-east to due north; but, so far as I know, not west of the north line. For these, at any rate for stations not far from the prolongation of the firing line, some totally different explanation must be found, and the first step must be to consider the effect of the wind.

Prof. Sir G. Stokes first pointed out in 1857 that, if the speed of the wind increases with the height above the earth's surface, the path of a sound ray cannot be straight, but must be curved and bent *downwards* towards the surface for rays *in the direction* of the wind, but *upwards against* the wind.

Now on February 1 there was a steady gentle wind, estimated at somewhere about five miles an hour, from the west with a cloudless sky, and no *visible* irregularities of cloud or fog to interfere with the normal propagation of sound. Hence for places west of the firing line the sound rays, on Stokes's principle, were diverted upwards into the upper atmosphere, and so passed over the heads of places at even moderate distances, so that no sound reached them. For places east of the same the contrary effect was produced, and closer examination is necessary to trace the consequences.

Proceeding from Stokes's principle, it is shown in Lord Rayleigh's "Treatise on Sound," ch. xiv., that on the hypothesis of the speed of the wind increasing upwards from the surface in direct proportion to the height, the path of a ray of sound advancing with the wind would be a catenary whose axis is vertical and directed downwards from its vertex, so that the ray, after ascending in its curved path to the vertex, would descend in a similar path again to the horizontal line through the point whence it started. Thus a ray starting from A (Fig. 2) in the direction AT would reach B in the horizontal line through A by the path ACB of the catenary, whose vertex is C, midway between A and B; and hence all the sound energy, which in air with no differential motion of its strata would have been spread over the circular sector TAB, must be concentrated between the curve ACB and the horizontal base AB. Now the angle TAB, when not large, is very nearly proportional to the distance AB, and hence in a vertical plane the concentration of the energy is proportional to the distance AB. On the other hand, in the horizontal plane the energy is spread out in azimuth proportionally to the distance AB, and thus the concentration in the vertical plane is balanced by the spread in the horizontal plane, so that the sound energy reaching two observers at different distances from A in the horizontal line AB would be about the same in amount.

There is, however, this difference, that, as the distance AB is greater, the difference in length between the curved path ACB and the direct line AB is greater, and this produces a greater retardation in time between the extreme curved ray and the horizontal ray, so that, the same energy being received in a longer time, the impression on the ear at a distant station would be enfeebled as compared with a nearer one.

Assuming this general reasoning to be correct, I have endeavoured to apply it quantitatively to the case of Eastbourne, or rather Beachy Head, for which we have the valuable observations of your correspondent, "H. D. G."

The firing line extended eastward passes directly to Beachy Head over a sea surface, with the exception of about two miles across the low peninsula of Selsea Bill. I have assumed the direction of the wind to coincide with this line, as it approximately did, and, farther, in the absence of definite data as to the rate at which the wind speed increased upwards, I have assumed it to amount to five miles per second for each mile of vertical height. I find then that at the distance of sixty miles from any firing point, which is about the distance of Beachy Head from Spithead, all sound rays starting at a less elevation above the horizon than about $11^\circ\ 23'$ would be bent round to the horizon between these two points, the extreme ray having reached an elevation of just over three miles; and the length of its path exceeding the direct horizontal distance by about four-tenths of a mile, it follows, from the known speed of sound, that the sound energy from a single gun would be at Beachy Head spread over an interval of very nearly two seconds. I conclude, therefore, that no single gun or pair of guns could have been heard there. But since the interval along the line between successive ships or pairs of ships corresponds only to a sound interval of just over one second, the sound impulses from successive ships would overlap (as they would not do at smaller distances, such as Chichester), and might so reinforce one another as to become audible. Doubtless the reflections of rays from the smooth sea surface would also contribute to this result.

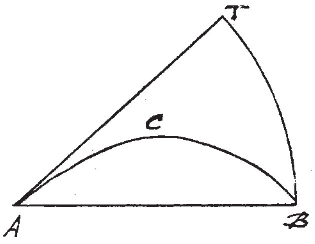

FIG. 2.

Of course, the assumed data are too hypothetical for any dependence to be paid to these figures as exact results, but I think they suffice to show that the observations of "H. D. G." may thus be fairly accounted for.

His description of the sound heard as "er-er-pup-pup" would correspond with the general roll and the explosion from a pair of guns in the double line not fired exactly simultaneously, while the duration which he notes of about eleven seconds for the whole sound would correspond fairly well with the interval, some eighteen seconds, by which the sound from the last ships of the double line would be behind that from the first, and would seem to prove that the sounds from the single line farther westward were inaudible.

It is well known that the speed of sound in air does not depend directly on the density, but is affected by temperature, so that in an atmosphere of varying temperature the course of a ray must be curved. In the normal condition it is stated that the fall of temperature is about 1° C. in 330 feet of vertical height, and the consequence is that every ray of sound must be deflected *upwards*, and so at a little distance from the source would pass to the upper regions of the atmosphere over the heads of observers on the same horizontal line. This effect is probably small compared with that due to the wind. It would add to the upward bending effect for rays proceeding against the wind (in our case westward), and somewhat flatten the curved paths with the wind; but it would not probably affect materially the general explanation above.

"H. D. G." truly observes that, owing to the curvature of the earth, his station was nearly 2000 feet below the horizontal line of Spithead, but this also would not materially alter our explanation.

So far for the sound heard at distant stations not very far from the east-west line from Spithead. When, however, we come to consider the case of Oxford and other places nearly due north, where the sound was heard, and in particular the duration of continuous sound (20 seconds) and succession of sounds and silences noted by Sir W. J. Herschel, our explanation entirely fails. It is true that the differences of distance from Oxford to the different ships were so small that the impulses from all the forty-six ships must have arrived within the interval of about half a second, but they would have diminished in intensity (according to the law of the inverse square of the distance, which in the absence of wind in their direction must be assumed) so as to be less strong than the impulse from a single gun at the distance of ten miles, a distance at which, we have seen, it would have been inaudible.

Was there a southerly current in the upper atmosphere between the Solent and Oxford? Or was the state of the atmosphere abnormal as to temperature, so that the upper regions were warmer than the lower, as hardly seems probable? Or, lastly, were the sounds heard by Sir W. J. Herschel diffraction effects outside the upward curving sound rays, as the intervals of sound and silence seem to suggest? Or is any other explanation possible from known laws?

Possibly some light may be thrown on these questions by other correspondents, or such experts as Sir G. Stokes, Lord Rayleigh or Prof. Osborne Reynolds.

The Cherbourg Peninsula at its northern end is about the same distance to the south of Spithead as Oxford to the north. It would be interesting to know if the sounds were heard by ships in the channel between the Isle of Wight and Cherbourg.

Robt. B. Hayward.

Ashcombe, Shanklin, March 12.

## The New Star in Perseus.

In sending you a provisional light-curve of Dr. Anderson's new star for publication in Nature, my principal purpose has been to ascertain the nature of the curious fluctuations in the latter part of its course. I have no doubt that they are real, as even the slight irregularity in the descending curve, about March 5, has been independently detected in Leyden, and probably elsewhere also; but the periodicity that seems to establish itself in the past six or seven days may be only apparent. In this country the weather has been generally unfavourable for some weeks, and it is possible that astronomers in other parts of the world will be able to fill the gaps between the observed parts of the descending curve (on February 25, March 1, 3, 5, 6, 13, 17, 20, 21, 22, 23, 25, 27).

This star is remarkable in still another respect. It is a well-known fact that new stars have almost exclusively made their appearance in the Milky Way; moreover, it has been pointed out recently by Sir Norman Lockyer in this journal that the Novæ are not equally distributed along the galactic zone; like the "Wolf-Rayet" stars, they seem to avoid the region comprised between Cassiopeia and Carina. Nova Persei 1901 is no exception to the general rule, it being situated on a feeble distance of the central plane of the Milky Way, but as in the case with the other new star discovered by Dr. Anderson, in Auriga, it lies in a relatively poor region of the galactic zone, in which phenomena of this kind have but rarely occurred. Notwithstanding this, I think that the tendency among the new stars to group themselves in the opposite region of the Milky Way holds good as a rule.

C. Easton.

Rotterdam, March 27.

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## NOVA PERSEI.

The observations of the new star in Perseus have not decreased in interest since they were last referred to in Nature. Strictly according to precedent a nebular spectrum, somewhat similar to that observed by Gothard in Nova Aurigæ, followed the disappearance of the dark lines in the spectrum; but about the same time a new phenomenon in relation to Nova was observed; the star behaved like a "collision-variable."

Sudden changes of magnitude have been one of the most interesting features of this new star. Since the time (February 23) that the Nova attained its greatest brilliancy, the star gradually diminished in brightness, decreasing rather rapidly till the 13th March, and somewhat more slowly up to the 17th. Since this date periodical variations have occurred, the star decreasing to a 5·5 magnitude star and rising to about 4·2 in a period of three days (about). Thus minima were observed on March 19, 22, 25 and 28. On the evenings of the 30th, 31st and April 1 the star was of mag. 4·2, 4·3, 4·4, so that either another minimum had occurred between the times at which these observations were made or the periodicity is undergoing a change. It is important, therefore, that the light of the Nova should be observed as often as possible, so that such changes may be accurately determined.

Whether this result is due to the complete capture of the denser swarm or to other changes brought about in the sparser one, it is as yet impossible to say.

We append some extracts from a paper communicated to the Royal Society by Sir Norman Lockyer last Thursday.

*Colour.*—The colour has undergone some distinct changes since the observation on March 5 last, when it was shining with a clarety-red hue. On the 9th and 10th it was observed to be much redder, due probably to the great development of the red C line of hydrogen.

On the 23rd and 24th the star was noted as yellowish-red, while on the 25th (after the sudden drop in magnitude) it was very red with perhaps a yellow tinge.

Since that date the star has again become much less red than formerly, and on April 1 was distinctly yellow with a reddish tinge.

*The Visual Spectrum.*—Since March 5 the spectrum has become much fainter, the bright lines of hydrogen being relatively more prominent than they were before; indeed, C and F throughout this period have been the most conspicuous lines, especially the former, while the bright lines λλ 5169, 5018 and 4924, and the line in the yellow at or near D, were the most prominent of the others.

All these lines have been gradually becoming weaker, but there is an indication that λ 5018 has been brightening relatively to λ 5169.

Accompanying the great diminution in the light of the Nova observed on the evening of the 25th, the spectrum was found to have undergone a great change: the continuous spectrum had practically disappeared, and a line near D (probably helium $D^3$) became more distinct. The other lines were hardly visible.

*The Photographic Spectrum.*—The spectral changes recorded in the photograph in one part of the spectrum follow suit with those observed visually in the other.

On March 6 the photograph was very similar to those obtained in the earlier stages, the only apparent difference being in the relative intensity of the bright hydrogen lines as opposed to those having other origins, most of which have been shown to be probably due to iron and calcium. The hydrogen lines have sensibly brightened, while the others have become much feebler.

The photograph of March 10 shows a further dimming of the bright lines other than those of hydrogen.

On March 25, when the next good photograph was taken, the spectrum had undergone great modifications. The hydrogen lines are still very bright, though they do not show the structure which they did in the photographs taken between February 25 and March 10. The bright lines other than those of hydrogen which are seen in the earlier photographs have now disappeared and other lines become visible. The continuous spectrum has also greatly diminished.

Rough determinations of the wave-length of these new lines have been made by Mr. Baxandall by interpolation between the hydrogen lines. They are as follows:—

387. Brood and merging into Hζ (3889).
436. Faint.
447. Not very strong. Probably Helium (λ4471·6).